seen by the eye. It would be better if these were put in as finely as possible, if included at all. The atlas is well up-to-date; and, owing to this fact, will probably be useful to the professional as well as to the amateur. The star places are marked for the epoch 1900, and the Harvard photometry has been taken as the authority for the magnitudes, the positions being derived chiefly from Argelander's Uranometria Nova. For observers possessed of instruments of moderate size, this atlas will probably prove a useful companion.

A Protest against the Modern Development of Unmusical Tone. By Thomas C. Lewis. Pp. 46. (London: Chiswick Press, 1897.)

THE prevalent practice in organ-building of the present day is to use for the middle C a pipe too large in scale, and with mouths cut too high, the result being, according to the author, that the Diapason tone, which rules every other stop in an organ, has deteriorated in quality. A pipe which will give an ideal Diapason tone is specified, and the defects in organs which do not conform to the conditions laid down are criticised. The protest as regards church bells is chiefly directed against excessive thickness. In pianofortes the destruction of pure tone is held to be due "to an increase of heaviness in the hammers for the pounding of the strings, to an excess of rigidity in the framework and setting, counteracting the vibrating motion of the strings—to an excess of scale in the length of strings—to the production of false harmonics, and the absence of due proportion between the groundtone and the harmonics, and generally to the making of more noise than music in the quality heard." The brochure contains some interesting information on the principles of the construction of organ-pipes, bells, and pianofortes.

Respiratory Proteids, Researches in Biological Chemistry. By A. B. Griffiths, Ph.D. Pp. v + 126. (London: L. Reeve and Co., 1897.)

THE conclusion which the author of this book aims at establishing is that there are several respiratory proteids (both coloured and colourless) in the blood of animals. The introductory chapter, occupying one-third of the pages of the book, brings together some interesting information on the constitution of the blood of echinoderms, annelids, insects, arachnids, crustaceans, molluscs and vertebrates. Following this are chapters on various respiratory pigments found in the blood of certain animals, and on colourless respiratory proteids. Chapters on the nature and functions of chlorophyll and hæmoglobin conclude the text. An appendix is devoted to brief descriptions of the chemical compositions of the chief pigments which occur in the bodies of animals, and the methods by which they may be extracted.

The book should be serviceable in directing attention to the comparatively neglected field of biological chemistry, even if all the views it contains as to biochemical processes are not accepted.

Outlines of Psychology. By Wilhelm Wundt. Translated by C. H. Judd. Pp. xviii + 342. (Leipzig: Wm. Engelmann. London: Williams and Norgate, 1897.)

THIS book differs from the other works of Prof. Wundt in being more purely psychological, the physiological aspect of the subject being kept as much as possible in the background. Like the other works, it is an exposition of the special attitude of the author rather than a critical account of the present state of knowledge on the subject; but this is a feature common to most psychological textbooks. For those who wish to learn the views held by the leader of one of the chief schools of modern psychology, the present volume will serve excellently. The translation is good, and Dr. Judd has added a useful glossary giving the German equivalents of the chief psychological terms used.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

Organised or Sectional Work in Astronomy.

THE remark was recently made by Prof. S. C. Chandler that, notwithstanding there had been no recent systematic arrangement of work in connection with variable stars, the result was most gratifying; for the observations were fairly complete, few interesting objects having been neglected. He says that "this satisfactory result could hardly have been reached so effectively by a formal organisation of work directed from headquarters, prescribing and circumscribing the operations of each participant, and destroying by its benumbing influence the enthusiasm which springs from the individual initiative of the observers themselves."

This statement emanating, as it does, from a thoroughly practical man, and being based on unequivocal facts, must commend itself to the consideration of every one interested or engaged in the sectional work of various societies. It is evidently a point worth inquiry, as to whether Prof. Chandler's remark applies with equal force to other departments of astronomy besides that of variable stars. Having had some little experience in the sectional work of the Liverpool and other astronomical associations, I may perhaps be allowed to express the opinion that, while in some branches there is great utility in co-operation, in others the material advantage is rather questionable. In comet-seeking the division of labour seems eminently desirable, because one observer cannot possibly examine all the available sky at sufficiently short intervals. In meteoric researches, also, concerted effort is most valuable for the purpose of securing duplicate observations. Amateurs, by pre-arranging the hours for simultaneously watching the heavens, and the particular region for each one to observe, are enabled to secure a number of observations of identical objects, and the real paths of these may be derived from the materials gathered in this way. If left to independent effort, the chances of success would be greatly diminished, and the accuracy of the observations impaired; for a person when engaged in special combined work is apt to put forth his best energies, and the appearance of a large meteor is not likely to find him unprepared, unless it comes at a time not included in the prescribed hours of work.

But, in some other departments of observation, there does not appear to exist the same necessity for organised effort. In fact, I think that it can be shown from results—the best of all tests—that it has been a comparative failure as far as it affects the progress of astronomy. Of course a great deal depends upon the director of a section. If he is a man of great resource and skill, he will be pretty sure to have something tangible to show for his work, and that of his colleagues. The worst of it is that, in publishing collective results, the good, bad, and indifferent are indiscriminately presented; and there being, perhaps, no criterion by which to distinguish them, the whole are virtually rendered useless. Taking any band of unselected observers those of moderate or poor capacity will greatly predominate. Even in meteoric astronomy, I would not, for an instant, recommend that the results of several observers should be combined with the idea of accurately determining the positions of radiant points. In such cases the bad or moderate observations swamp the trustworthy ones, and we can get radiants anywhere or nowhere, just as we like to interpret the evidence afforded by the materials before us. It is a most important requirement that really precise observations should be preserved from contact or collaboration with others of inferior character.

A little reflection will prove that all the best work has been accomplished by individual and independent effort. A good man will persevere in his labours, just the same, whether he belongs to any combination or not; and it is really much better for such a person to be isolated, so that he may perform the work of his choice in his own way, and publish it in his own style. If a man has the ability to accomplish useful work, he will know the best form in which it may be presented for the benefit of science. Moreover, he needs no encouragement; he proceeds with his research because he is actuated by the love of it, and sees the beacon of success shining invitingly in the foreground.

Undoubtedly, cases could be cited where combined work has been or will be most efficacious. In an object of exceptional

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kind, like that involved in the preparation of the photographic chart of the heavens, it was absolutely necessary, from the magnitude of the undertaking, that a collective effort should be made. In another case, that of the British Association Committee on Luminous Meteors, which existed between 1848 and 1881, a mass of valuable work was performed (as the annual reports will testify) by the collection and discussion of observations and investigation of theories. Other instances might be adduced, but they are rather exceptional in character and distinct to the ordinary sectional work of societies.

In certain respects, it cannot be denied that the latter serve a useful purpose. Many gentlemen find it an encouragement and a source of interest to engage with others in combined work. They are thus enabled to compare notes, and it is a satisfaction to feel that a bond of association exists between them, and that they are all actively employed in a similar direction. Observations are taken, drawings are made, and many hours are spent at the telescope, which would never be so employed but for the influence of the circumstances referred to. They have the pleasure of seeing their observations in print; possibly some of their drawings are also reproduced, and the consciousness of having done something to merit public notice cannot fail to stimulate them to further effort. But, in such cases, it must be admitted that the benefit to science is inconsiderable. Very little work of real value is accomplished in this way, and in many instances the observations are not properly reduced and utilised as they should be. It is not sufficient that results of this kind should be simply allowed, year after year, to accumulate. Many thousands of drawings and observations have been made by the members of planetary sections; but we can trace very few salient facts, or additions to our knowledge, as the outcome of them all. Observers, as a rule, do not probe into their subject with sufficient depth, and ferret out all the details possible of any particular object observed. Nor is attention always directed to those points which are the most significant and suggestive. It needs a man like Mr. Marth to be the really efficient director of a section, to single out the really essential work to be performed, and then to sift it with thoroughness and critical accuracy.

To beginners sectional work is often most beneficial, as it affords them a useful preliminary training. But observers who need and will submit to "direction," except at the outset of their careers, are not generally the men who accomplish work of an important and enduring kind. The aspirations of a really capable man are not likely to be satisfied by the facilities offered by combination with many others. It has been said, "Talent does what it can, Genius does what it must." When a young observer begins to feel confidence in himself, it is, perhaps, better that he should strike out in a path of his own. There are some who will naturally be allured by the prospects of doing original work, and effecting discoveries in an independent way. They do not want to triple the channels of Mars, to distinguish the hard straight lines on Mercury and Venus, or to trace the zebra-leopard-like aspect of the globe of Saturn. But they want to do really useful work, and to rely only upon the unmistakable evidence of their eyes; in this respect, dissociating themselves from some modern observers, who can but very vaguely discriminate between romance and reality.

To sum up the matter: it appears that the organised work of "sections," though it unquestionably affords a stimulus to many, and assists in maintaining the interest in a subject, is yet, except in certain special circumstances and cases, disappointing and unproductive of results which materially advance astronomy. Individual and independent effort has hitherto been, and will still continue to be, the fountain-head of the most valuable work.

In concluding, it may be mentioned that the issues of recent planetary observation appear to be totally dissimilar to anything previously experienced in astronomical history. No two observers see alike when they examine the images of Mercury, Venus, Mars, or Saturn, and the actual character of the visible surface markings of these orbs is more an enigma than it was in the days of Herschel and Schroeter. There is also a pronounced conflict of opinion as to the utility of large and small telescopes in displaying delicate features on the planets. This want of unanimity amongst observers has become a serious question to consider; in its presence organised attempts to study the planets are of little avail, since many individuals seem to display their own particular idiosyncrasies and peccadilloes, greatly to the chagrin of every director of a section, who finds his post no sinecure.

W. F. DENNING.

Shelly Glacial Deposits.

I FEAR that the hope expressed by Prof. Bonney, somewhat incongruously in its connection, in his recent review of Russell's "Glaciers of North America," that "perhaps in future we shall hear less of rampant ice-sheets at Gloppa and Moel Tryfan!" is not destined to be fulfilled. There will be something more to hear shortly, if he care to listen, respecting that part of this ice-sheet which covered the Isle of Man. This portion was distinctly of the "rampant" type, as Mr. P. F. Kendall has already shown, carrying up shells in one place, and boulders of Foxdale granite in another, and erratics from the south of Scotland in another, as a matter of every-day work—just as recent investigations have shown to be the case in regions where to-day there are glaciers of other than the Alpine type.

I am quite in agreement with Prof. Bonney when, elsewhere in his review, he asks: "May not the difficulties of the subject be augmented by defective knowledge?" For this reason I may be pardoned for once more dragging forward the facts which I put on record some years ago respecting the shelly Basement Clay of the Yorkshire Coast. In this deposit the shells occur not only scattered throughout the clay, but also in limited patches or boulders of marine sand and mud, which are associated with similar masses of peat and mud of fresh-water origin, and with patches of shale and clay derived from the Lower Cretaceous and Jurassic strata of the country farther northward with the bedding still preserved and the characteristic fossils in place.

These facts have never been impugned, but they are rarely referred to by the opponents of the "rampant ice-sheets." They have surely a more immediate and direct bearing upon the subject than the isolated observation respecting the deposit in the neighbourhood of the Malaspina Glacier on which Prof. Bonney leans so wide a hope.

If the sands and gravels accompanying this Yorkshire drift-series be, as is usually held, the result of the washing-out of the same material, the shelly fragments contained therein are no better proof that the gravels are of marine origin than their derivative Jurassic fossils are that they are of Jurassic age.

I do not think that any one has attempted to deny that marine deposits of Glacial age may and do exist within the limits of the British Islands. But what the "extreme glacialists" wish to insist upon is that better evidence is required than the mere presence of sporadic marine organisms to prove such origin against the very strong evidence which can be adduced against it in such instances as those referred to by Prof. Bonney.

Dalby, Isle of Man, April 22. G. W. LAMPLUGH.

Sieve for Primes.

MAY I draw the attention of your readers to a series from which the primes may be recovered?

The series is given below, together with the accompanying primes.

1,	4,	11,	29,	76,	199,	521,	1364,	3571,	9349,	&c.
1.	3.	5.	7.		11.	13.		17.	19,	&c.
		2.								

The law of formation is $a_{n+1} \equiv 3a_n - a_{n-1}$.

It can be proved in various ways that the nth term of

$$(w_2 + w_3)^{2n-1} + (w_4 + w_5)^{2n-1} - 1 \equiv p.q$$

where the roots are the unreal of $x^5 + 1 \equiv 0$ and $p = 2n - 1$ is any odd prime.

Is 13 a prime? Yes; because the 7th term $(2 \times 7 - 1 = 13)$ minus unity $= 13\,q$.

Is 15 a prime? No; because the 8th term less unity is not $= 15\,q$.

These are but easy numbers to test; but the law is general. We have here an alternative test for primes.

The series given above is intimately connected with the well-known "continuant" series 1, 1, 2, 3, 5, 8, 13, &c., whose law of formation is obvious.

The connection between the two series is as follows:—

Let a, b, be any two consecutive terms of the "continuant" series.

Then $5\,ab \pm 1$ will give the corresponding term in the former series.

There are other series which produce the primes, but the above can be produced *mechanically*.

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